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A mesoscale forecast for 14 August 1975—the Hampstead storm

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Summary

A three-dimensional numerical atmospheric model has been used to obtain a forecast for the afternoon of 14 August 1975, when an intense stationary multicell storm formed over London and produced extensive flooding. The forecast shows a well-defined area of convergence at the right place, but about four hours later than the actual storm. Shading by an observed layer of medium-level cloud is included in the forecast model, and it is clear that this has an important effect on the forecast.

1. Introduction

On 14 August 1975, a stationary storm centred over north-west London caused extensive flooding. The natural history of this storm, the Hampstead storm, has been well rehearsed (Keers and Wescott 1976, Atkinson 1977) and the dynamics of the storm itself have been studied using a numerical model (Miller 1978). With this background, this situation is a natural choice as a case-study for testing a mesoscale forecast model. Such a model should provide detail that is not achievable in a synoptic-scale model and thus help to elucidate or forecast the environment in which strictly local events like the Hampstead storm might occur. Tapp and White (1976) have described a model that is designed to give mesoscale weather forecasts for the British Isles, and this model has been used to attempt an experimental forecast for 14 August 1975.

A limited-area atmospheric model needs initial conditions, which will describe the synoptic situation as well as any mesoscale detail that can be inferred from observations, and boundary conditions, through which information about synoptic changes can be supplied. In the present study, no attempt has been made to use observations to improve the initial conditions, which were interpolated from a synoptic-scale model. In this respect, and in many others, it is exactly like an earlier case-study reported by Carpenter (1979). This simplification was made because we do not yet have the ability to calculate mesoscale initial conditions automatically, and it was not thought appropriate to go to the considerable effort of carrying out and digitizing subjective analyses. The boundary conditions were also calculated from the synoptic-scale forecast for the period in question.

The model is still being developed and does not include any treatment of clouds. Fortunately, the most obvious feature of the cloud cover was a bank of altocumulus spreading from the west. With the exception of small areas in the west of the country, once this cloud arrived at any place it remained cloudy there for the rest of the day. Thus it was possible to allow for the effect of this cloud on the surface heat fluxes by coding its time of arrival at each grid point. Two forecasts, with and without this

cloud, were carried out and we shall argue that the effect of the cloud is so marked in the forecast that it must be relevant to the mesoscale dynamics of the storm, and that cloud shading is an important part of any reasonable local weather forecast model.

2. Observational background

The synoptic surface-pressure analysis for 12 GMT on 14 August 1975 is shown in Fig. 1, which also shows the positions of Hampstead and Crawley. The radiosonde ascent at 12 GMT from Crawley is shown in Fig. 2.

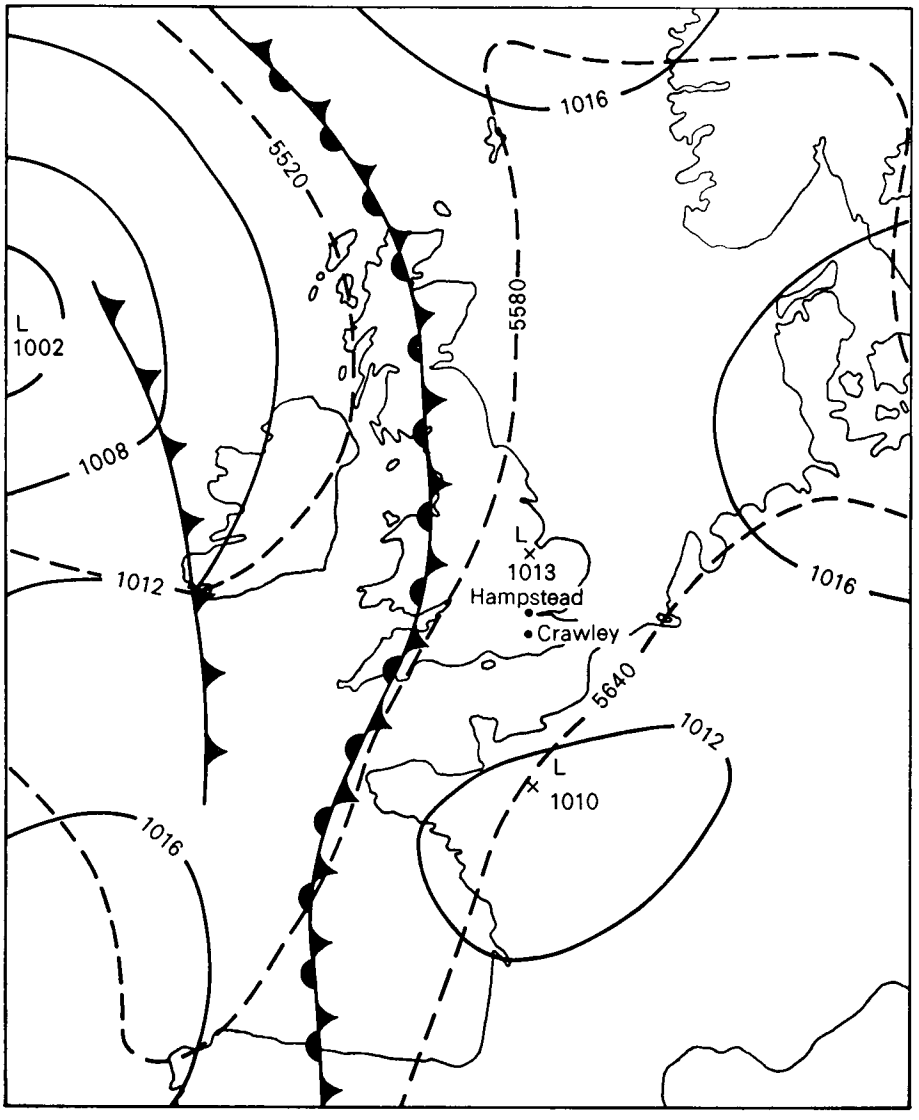

Figure 1. The synoptic situation over the British Isles at 12 GMT on 14 August 1975.

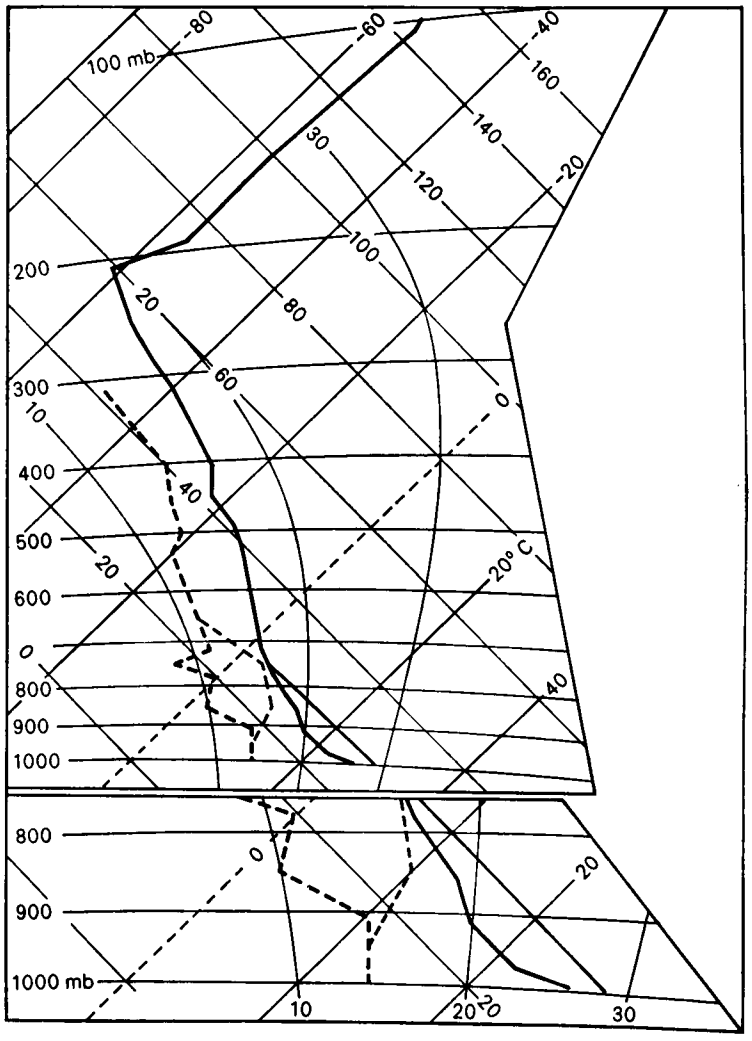

Figure 2. Radiosonde ascent from Crawley in south-east England at 12 GMT on 14 August 1975. Modifications that Miller (1978) found necessary in order to simulate the Hampstead storm are shown by the lighter variant lines.

It is natural to ask why a storm developed in this environment, why it was stationary and why it was over London. The ascent is conditionally unstable and south-east England is in the confluence between air moving round the depression to the north-west of Ireland and south-easterlies circling the low-pressure areas over France; the storm at Hampstead was not the only thunderstorm on this day. Miller (1978) has demonstrated that the storm was a multicell storm, in which each cell moved northwards in the ambient southerlies. The cold outflow from the succession of cells spread rapidly to the north-west, but the southern edge of the cold air was more or less stationary. Warm air from the south was forced over this cold air, triggering the release of latent heat and the growth of further cells. The initial development of the storm over Hampstead has been ascribed to Hampstead Hill, or to the urban

heat-island effect of central London (Atkinson 1977). Miller's work does not resolve this problem, and it might be asking too much to expect to be able to identify the precise perturbation amongst the many that could have precipitated a storm of this nature. The main reason for carrying out the mesoscale forecast reported here is to discover whether it would have been possible to give warning that the London area might be a preferred location for severe thunderstorms.

As part of his study of London's urban heat-island effect, Atkinson has produced mesoscale streamline analyses for the surface on 14 August 1975. These are reproduced in Fig. 3 so that they can be compared with the present forecast.

3. The model

The model has been described in detail by Tapp and White (1976) and Carpenter (1979). It is based on a finite-difference approximation of the non-hydrostatic compressible equations of motion on a three-dimensional grid. The horizontal grid has 61 × 62 points, a grid length of 10 km and covers England and Wales. There are 10 levels and the top level is at 4 km. This grid is not a permanent feature of the model and it might have been natural to increase the model depth for this study. However, that could only be done at the expense of resolution and, in the event, the results suggest that there would be little benefit from using a deeper model.

Boundary-layer turbulence is included in the model, using a K-theory approach, and there is a convective adjustment. However, the effects of moisture, cloud and radiation are only described in so far as they affect the exchanges of heat at the surface.

The calculation of surface fluxes of heat and momentum has been described by Carpenter (1977). Monin–Obukhov similarity theory is used, and the surface temperature is given by requiring heat balance at the surface:

$$S + R\downarrow = H + LE + G + R\uparrow, \qquad (1)$$

where S is the solar heating of the surface,
$R\downarrow$ and $R\uparrow$ are the thermal radiation fluxes at the surface,
H is the sensible heat flux into the atmosphere,
E is the flux of water vapour into the atmosphere,
L is the latent heat of vaporization,
and G is the heat flux into the ground.

The calculation of the evaporation E needs a value for humidity mixing ratio, which is otherwise absent from the model, at the bottom level; a constant value of 0·01 has been used. The solar radiation S is given by:

$$S = 558\cos(t/12) + 72 \quad (\mathrm{W\ m^{-2}}) \qquad (2)$$

before the arrival of the medium cloud and

$$S = 279\cos(t/12) + 36 \quad (\mathrm{W\ m^{-2}}) \qquad (3)$$

later in the day, where t is measured in hours from midday. Thus the solar heating is halved following the arrival of the cloud. Equations (2) and (3) are intended to allow for atmospheric attenuation and reflection from the surface.

The facts that humidity was not a forecast variable and that the altocumulus was not the only cloud observed during the day reduce the reliability of the estimate of the sensible heat flux. Thus, it seemed inappropriate to consider the precise form of equations (2) and (3) too carefully, and the reduction of

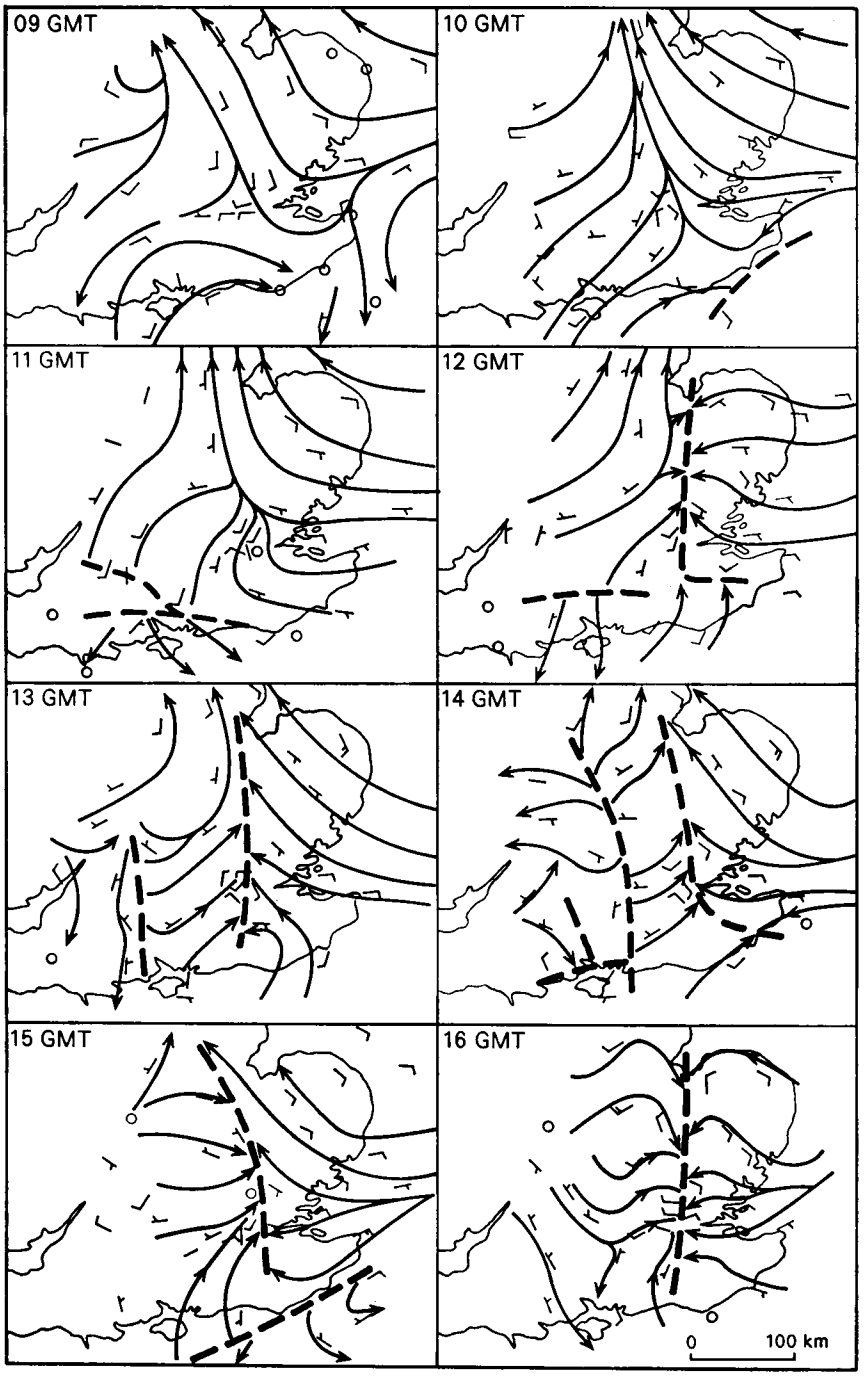

Figure 3. Surface streamline analyses for 14 August 1975. This figure is a direct copy from Atkinson (1977).

the solar beam by 50% was chosen for its simplicity. If we assume a transmissivity of 0·3 for medium-level cloud (e.g. Liou 1976), a solar beam S will be reduced to $(1-0{\cdot}7C_m)S$ by a cloud amount C_m. The cloud was recognized by the British Isles forecaster in the Central Forecasting Office, Bracknell, as a layer of more than 5/8 altocumulus, so about 55% of the solar beam should have penetrated the cloud. Thus equations (2) and (3) should give a qualitatively correct description of the shading effect of this altocumulus.

The leading edge of the cloud, as coded in the model, is shown in Fig. 4.

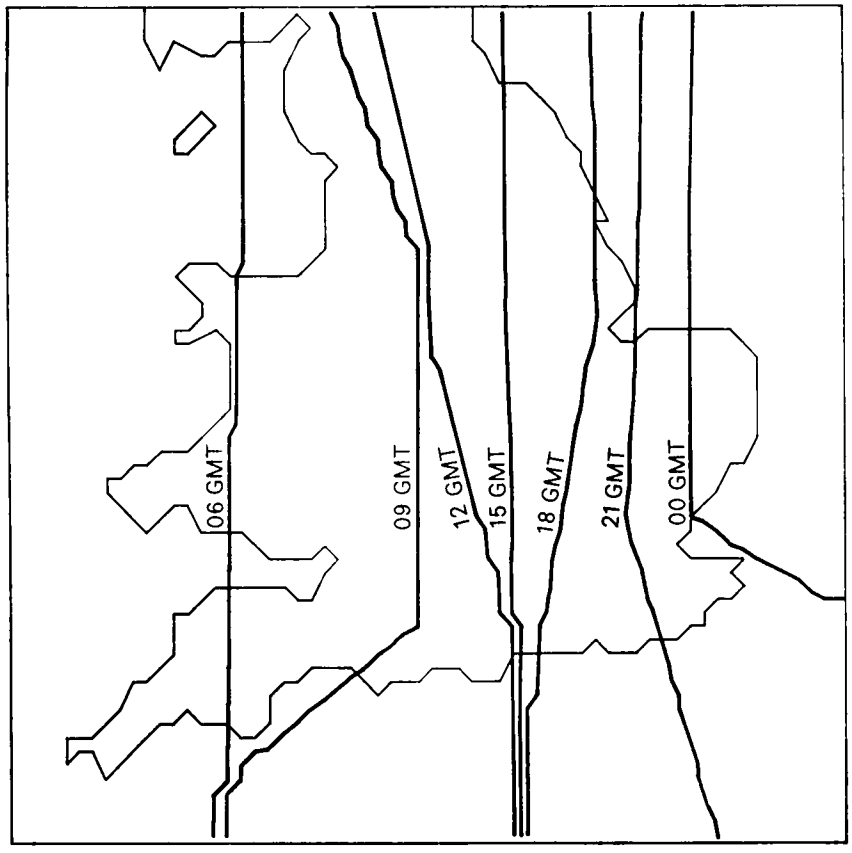

Figure 4. The position at various times of the leading edge of a layer of cloud that moved across England on 14 August 1975.

A synoptic-scale model starting at 00 GMT on 14 August 1975 was used to obtain initial and boundary conditions. The mass field forecast for 06 GMT was interpolated to the mesoscale model grid, and the Ekman layer equations were solved for the winds. The boundary conditions were updated throughout the mesoscale forecast using a second 'initialization' carried out for 18 GMT.

4. Results without cloud

A forecast was made using only equation (2) to calculate the solar warming of the surface. This gave the winds and temperatures shown in Figs 5 and 6 for 12 GMT and 18 GMT respectively.

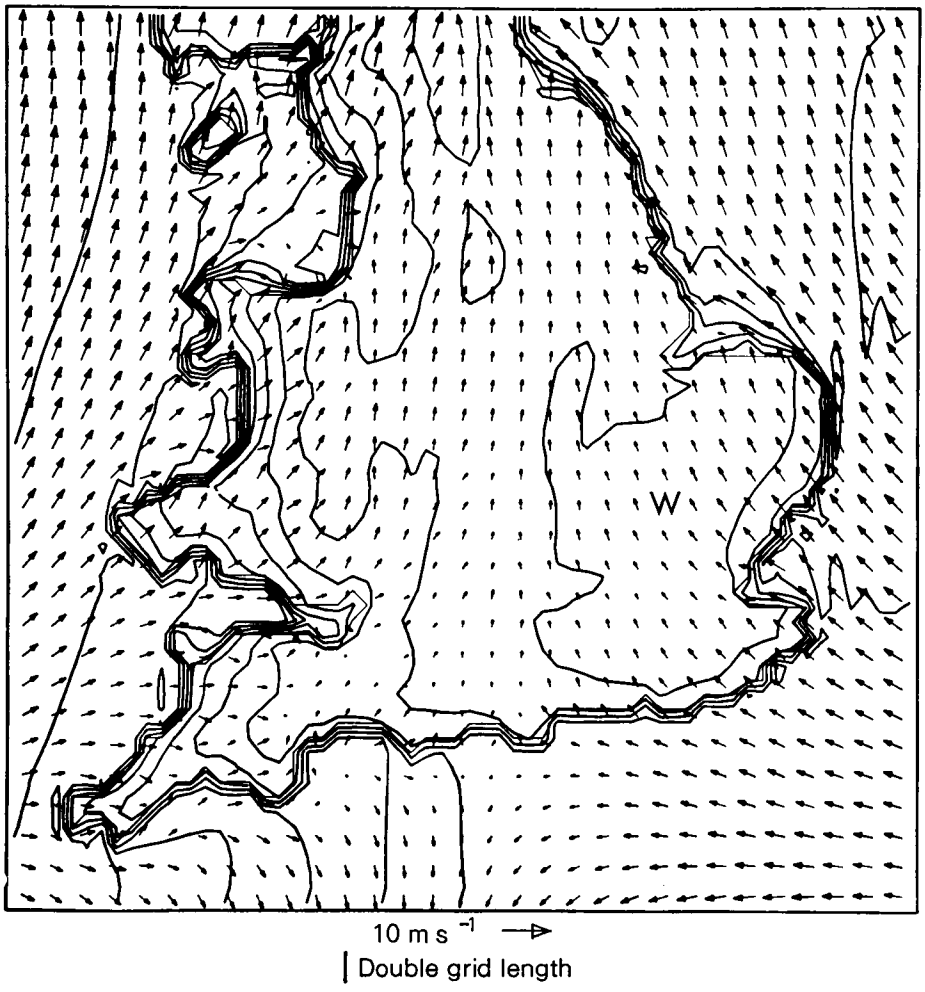

Figure 5. Forecast 10 m winds and potential temperatures for 12 GMT on 14 August 1975, when cloud effects were excluded from the forecast model. Wind speed indicated by length of arrow and plotted at double grid-length intervals. Isotherm interval 1 K; W = warm.

Given ascents resembling the Crawley ascent shown in Fig. 2, it is possible that the sea-breezes apparent in Figs 5 and 6 will trigger cumulus. Further, the convergence of breezes from the south and east coasts might indicate London as a preferred location for cumulonimbus. Nevertheless, it seems more likely that the results with cloud, shown below, are relevant to the special events on 14 August 1975.

5. Results with cloud

Figs 7 and 8 show the forecast of low-level winds and temperature for 12 GMT and 18 GMT, and are

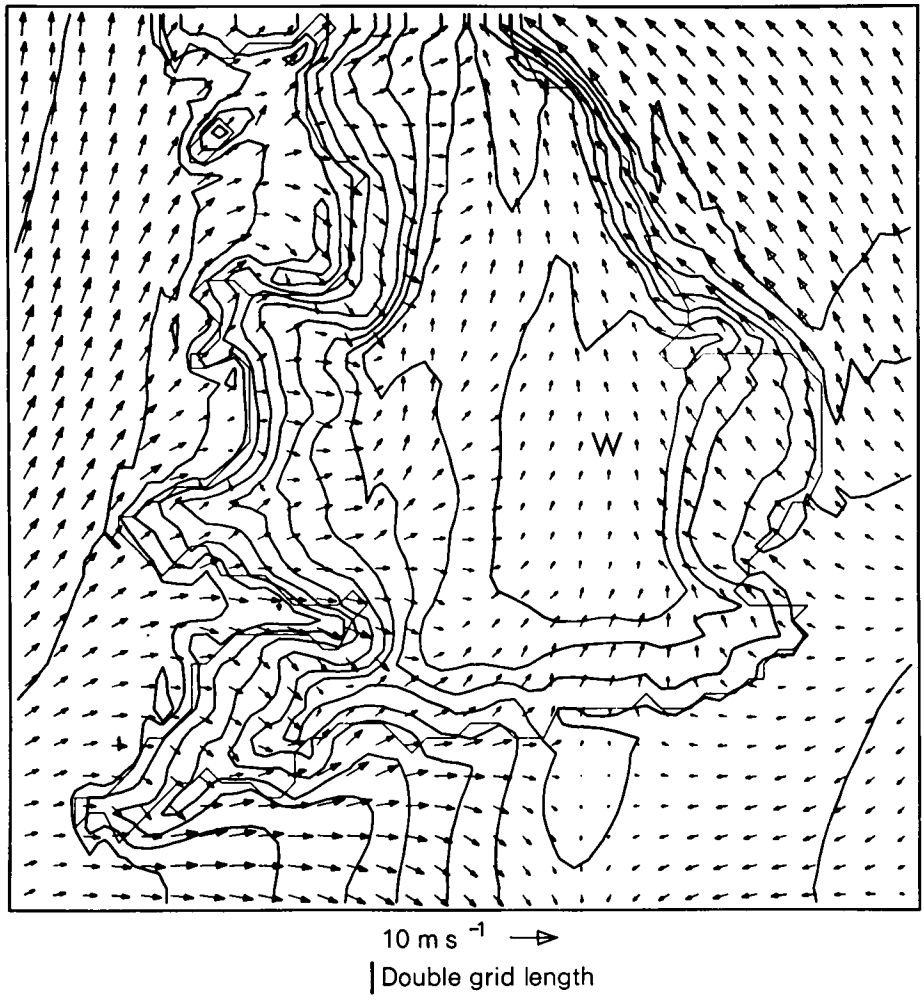

Figure 6. Forecast 10 m winds and potential temperatures for 18 GMT on 14 August 1975, when cloud effects were excluded from the forecast model.

directly comparable with Figs 5 and 6. The forecast for 18 GMT encouraged us to continue the forecast to 20 GMT, with the result shown in Fig. 9. Fig. 10, which shows forecast ascent and reports of thunder or cumulonimbus, shows Hampstead to be at the southern end of a line of ascent, and Fig. 9 shows a well-defined vortex in the same place. Superficially, this is a remarkable success, but it must be borne in mind that the storm started at 16 GMT, so this forecast is four hours late.

Fig. 11 shows a comparison between the forecast surface temperatures and the observed screen temperatures. In general, the surface temperatures are about 1 K too high, which might be reasonable for the decrease in temperature from the surface to screen level. The sharp discontinuity across the cloud edge is not proved by the observations, but the drop in temperature from the eastern half to the western half of England seems correct.

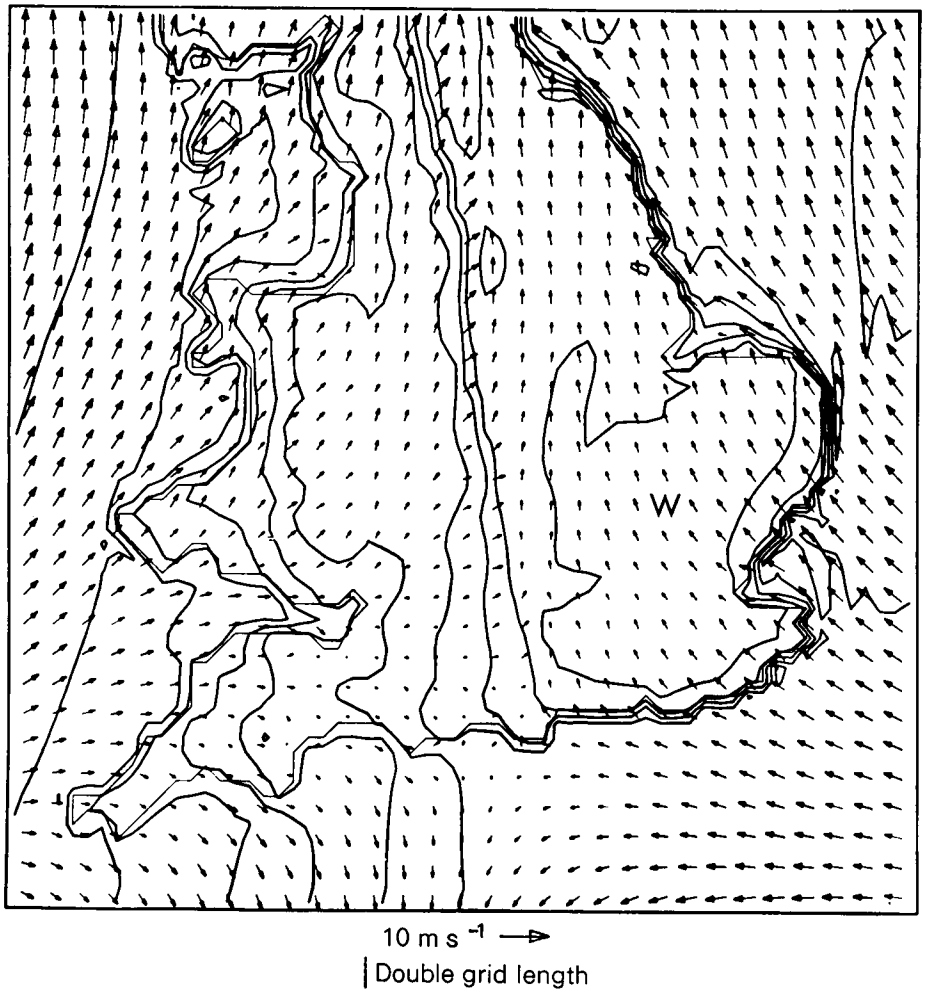

Figure 7. Forecast 10 m winds and potential temperatures for 12 GMT on 14 August 1975 when the effects of the layer of cloud shown in Fig. 4 were included in the forecast model.

6. Discussion

When the forecast with cloud is compared with the analyses in Fig. 3 it can be seen that some details of the forecast are quite wrong. In the analysis a line of divergence moves across the country in a way that is similar to the movement of the cloud edge and its associated convergence line in the forecast. The forecast shows divergence behind the convergence line and it is possible that a more realistic description of the cloud, with the amount increasing more gradually towards the west and the inclusion of some higher cloud further to the east, would lead to a forecast more like Atkinson's analysis. Atkinson's line of convergence is aligned along the east of the country far earlier than any similar feature in the forecast, and it seems unlikely to us that this has much to do with the advancing cloud parametrized in the forecast.

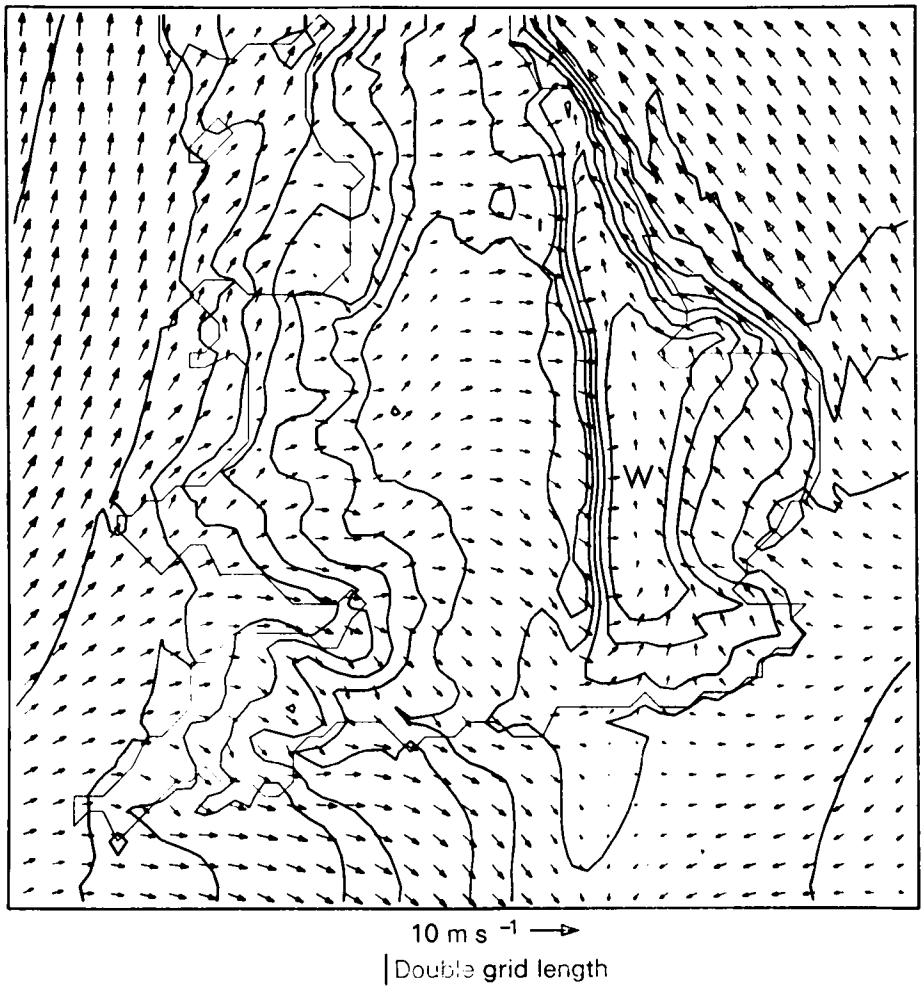

Figure 8. As for Fig. 7 but for 18 GMT.

Nevertheless, the comparison between the forecasts is conclusive evidence that the effects of the cloud are important, and the location of all the thundery activity close to the leading edge of the cloud (see Fig. 10) cannot be dismissed as a coincidence. Fig. 12 shows a west–east section through the model, and thus the cloud edge. It is clear that the cloud shading has induced a considerable temperature contrast throughout the boundary layer. This has in turn induced a thermally direct circulation in some ways similar to a sea-breeze or gravity current. It is hoped that this response will be studied further, and a further case-study in a similar situation is planned (the Skipton storm, 13 June 1979).

It seems clear that any reasonable local forecast model must include calculations of the shading effect of clouds. The importance of calculating the latent heat release during cloud and rain formation is familiar, but it is possibly no more important than the radiative effects of cloud. This presents modellers with a substantial problem because it is not clear that it is easy to calculate cloud cover or cloud type with reasonable accuracy in a numerical model. In this forecast we have, in effect, treated the cloud

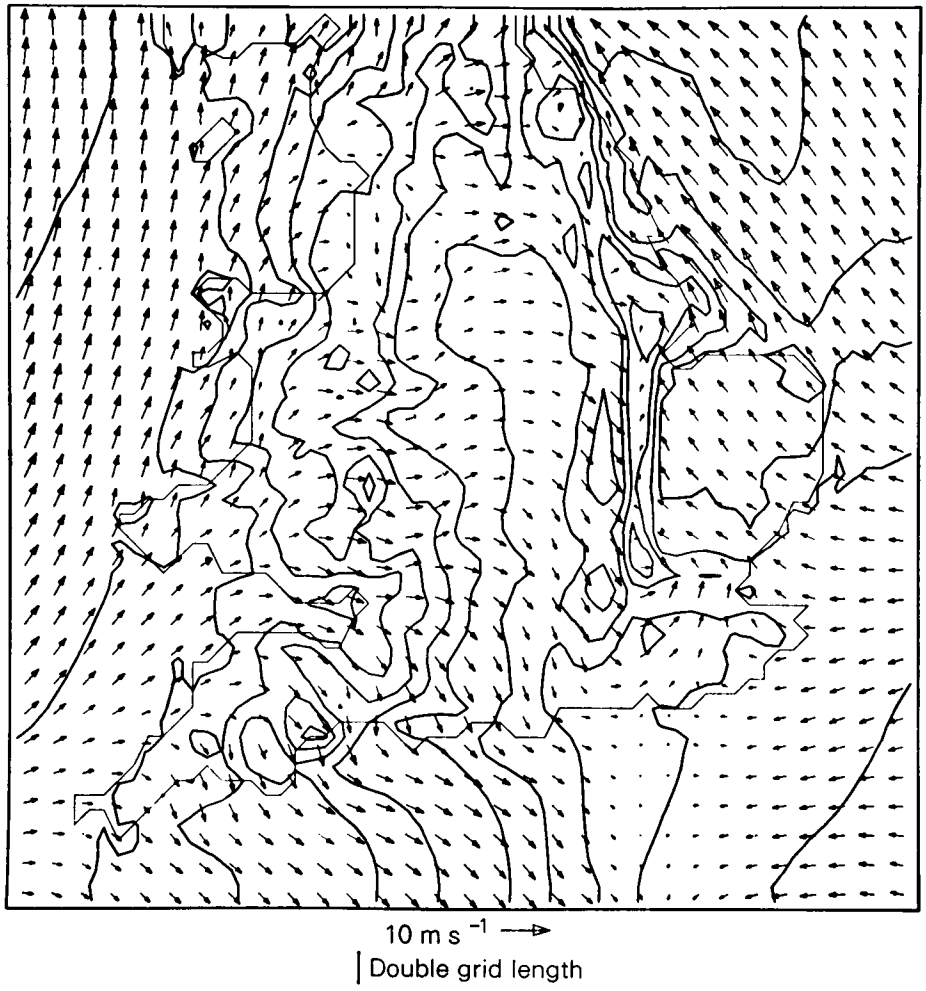

Figure 9. As for Fig. 7, but for 20 GMT.

amount as a boundary condition that must and can be obtained from some external source. In operational practice, we would not have observations of clouds in advance, but it is possible that forecasts of cloud type and amount could be obtained using some objective extrapolation of satellite and surface observations. This suggests that an approach like, but more sophisticated than, that used in the present study might be more reliable than using model-predicted cloud in the radiation calculations.

As for the Hampstead storm, it is clear that no description of its mesoscale meteorology is complete unless it takes full account of the effects of cloud shading and the associated areas of ascent and descent. Our forecast cannot be considered a complete explanation because of the lack of mesoscale detail in the initial conditions, the timing error and the poor comparison with Atkinson's streamline analyses. However, Miller (informal contact) has pointed out that the strong low-level convergence predicted in the forecast reported here is a useful addition to his work (Miller 1978). He found that the Crawley ascent (Fig. 2) did not lead to a storm in his model unless he increased the low-level heat and moisture, as shown in Fig. 2. His adjustment to the Crawley ascent is made more plausible by our forecast.

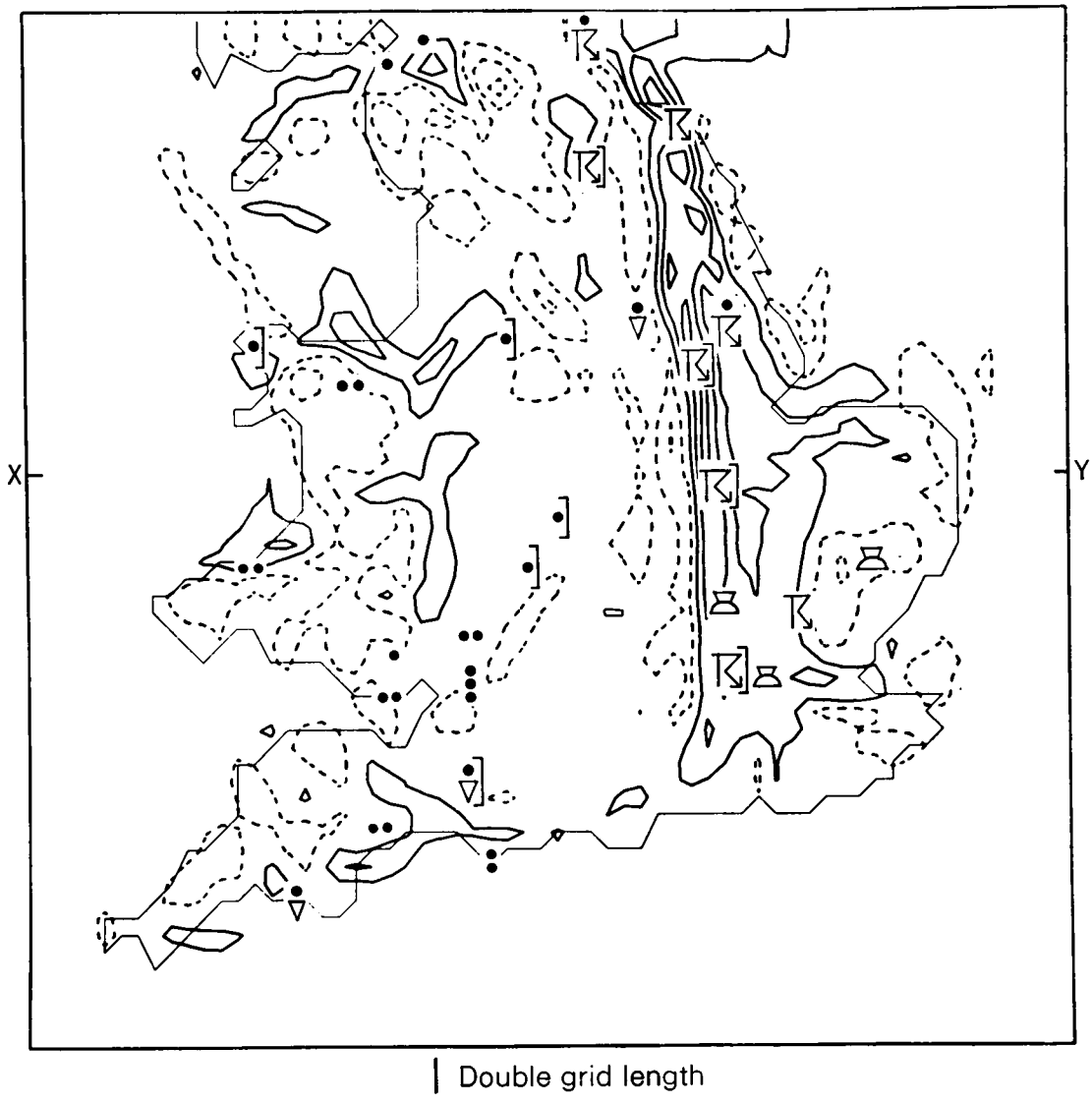

Figure 10. Forecast vertical velocity at 190 m compared with significant-weather reports, both for 18 GMT on 14 August 1975. Isopleth interval 2 cm s^{-1}; continuous lines +1, +3, . . ., dashed lines −1, −3, ... X and Y indicate the cross-section depicted in Fig. 12.

7. Conclusion

Two forecasts of the mesoscale developments on the day of the Hampstead storm have been carried out, one with and one without cloud. The comparison of the two forecasts suggests that the calculation of cloud shading is essential to good numerical forecasts of local weather. The forecast with cloud showed marked low-level convergence on the leading edge of the cloud, which moved across the country and eventually intersected the sea-breeze fronts to suggest a preferred area for convection over London.

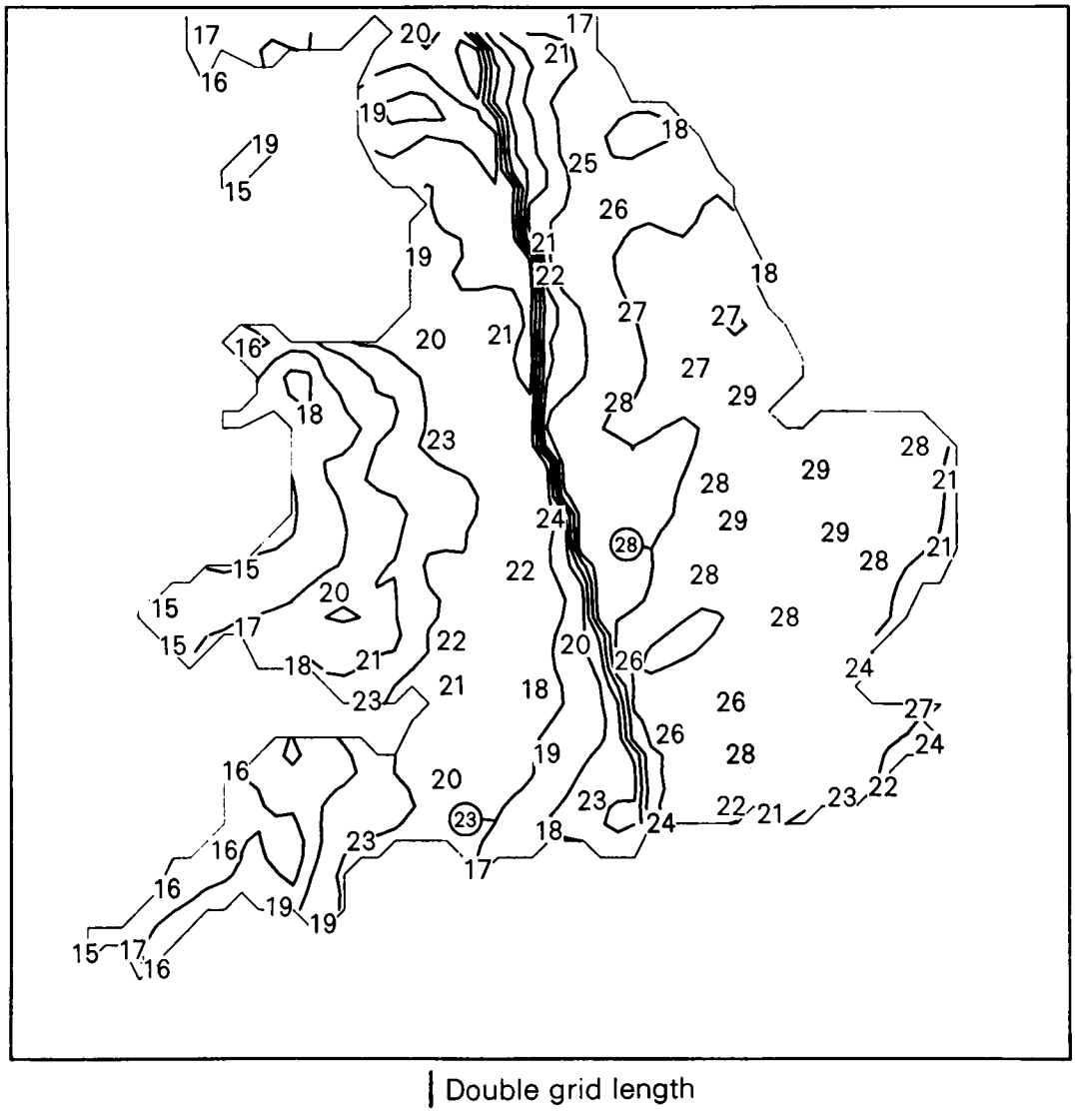

Figure 11. Forecast surface isotherms compared with observed screen-level temperatures, both for 12 GMT on 14 August 1975. Isotherm interval 1 K.

Acknowledgement

We are grateful to Drs M. J. Miller and M. W. Moncrieff for suggesting this study and for taking an interest in it.

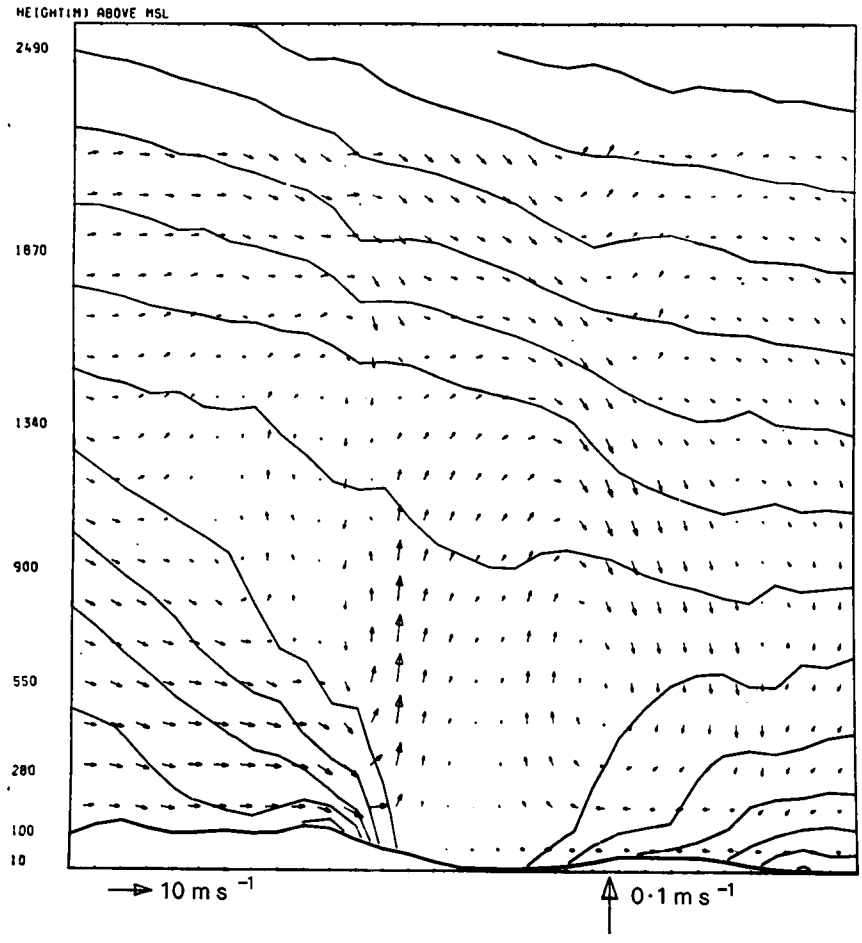

Figure 12. Vertical west–east section (X–Y in Fig. 10) through the cloud edge showing the forecast potential temperatures and components of wind velocity in the plane of the section for 18 GMT on 14 August 1975. The section is restricted to the bottom eight levels of the model and, approximately, to the eastern half of the model. Isotherm interval 1 K.

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Early meteorological observations for Sitka, Alaska

By D. E. Parker

(Meteorological Office, Bracknell)

Summary

Work has begun on the compilation of homogeneous time-series of surface data for Sitka, Alaska, where observations have been made, with a few breaks, since 1833. This note describes the solution of the basic problems of station elevation and observing times for the two series of data covering 1833–42 and 1842–67.

1. Introduction

Early meteorological data for Sitka, Alaska, remain among the untapped resources for research into climatic change. This note describes the start of efforts to render the data suitable for use in this research.

Sitka is at approximately 57° 3'N, 135° 21'W and is in that narrow coastal strip of Alaska facing west-south-west on to the Pacific north of Vancouver. It is situated partly on the south-west coast of the mountainous Baranof Island and partly on Japonski Island, which is separated from Baranof Island by a channel 300 m wide, used as a harbour. No part of Japonski Island exceeds 25 m in elevation.

The early meteorological data are summarized by the USA (Washington) *Pacific Coast Pilot* (1879), but without solving the problem of station elevation. However, following the recent discovery of evidence on the elevation of one of the two stations whose periods overlapped in 1842, it has become possible to deduce the other by comparison of surface pressures as described below.

2. Comparison of two stations operating simultaneously in 1842

From March to November 1842 there were two meteorological stations in Sitka. The four-times daily observations of the priests Veniaminoff and Cygnaeus from 1 January 1833 to 11 December 1842, new style calendar, are given by Kupffer (1846). Their station will be denoted station A. The hourly observations of the Russian Government are given by the *Annuaire Magnétique et Météorologique de Russie* from 1 March 1842 to the end of 1845, and thereafter by the *Annales de l'Observatoire Physique Central de Russie* until the station closed when Alaska was sold to the USA in 1867. This station will be denoted station B.

The individual data for a given time at stations A and B appear to disagree grossly, but this is because, according to Kupffer (1846), the times given are local time for station A, and Göttingen time (minus 12 hours because 01h etc. appear to be at night from an inspection of temperatures) for station B. Sitka is 9 hours 40 minutes west of Göttingen, so a discrepancy of 2 hours 20 minutes might be expected, but Kupffer in his conversion from the old style calendar decided to reckon time eastwards from Europe, so the discrepancy is in fact 1 day 2 hours and 20 minutes. Table I illustrates the times used. The discrepancy in the ascribed times has been verified by computing the apparent time differences for which

Table I. *Times ascribed to Sitka data*, 1842

Sitka Station A (Kupffer, 1846)	Sitka Station B (Russian vols)	Göttingen (Germany)	Greenwich
date/time	*date/time*	*date/time*	*date/time*
02/1500	01/1240	02/0040	02/0000
02/2100	01/1840	02/0640	02/0600
03/0300	02/0040	02/1240	02/1200
03/0900	02/0640	02/1840	02/1800

the variance of the pressure difference between the two stations is a minimum. This time difference was found to be about 1 day 2 hours, with variation of an hour either way according to which month of 1842 was chosen, the scatter probably arising from random errors.

When the truly simultaneous data are compared, after correcting Kupffer's observations to a barometer temperature of 13⅓° Réaumur, it is found that the mean pressure difference is 0·8 Russian half-lines = 1·4 mb, representing (at 7 °C) an elevation difference of 12 m, with station A having higher elevation (lower pressure). The USA National Weather Service (private correspondence) have found that station B had an elevation of 35 feet = 11 metres above mean sea level. Therefore station A must be taken to have had an elevation of 23 m.

De Tillo (1890) who used the Russian annals for his source, and therefore referred to station B, gives 19 m for the elevation. However, these annals, wherever they refer to the location of the station, consistently leave the elevation blank. Moreover, Washington (1879) shows a line drawing of station B which is described as being at the eastern end of the low and flat Japonski Island, and a recent Admiralty map of this island shows no elevation approaching 19 m on its eastern side (even allowing for the height of the barometer above the ground: the station building had no upper storey). Therefore the value 11 metres, and not de Tillo's value, will be used for station B.

3. Future work

Appendices 1 and 2 give further information about Sitka distilled from Kupffer (1846) and from the Russian annals. It will be necessary to check and if necessary eliminate the suspect values, and to make the early data as homogeneous as possible with later data for which the station elevation is known. A little extra insight may then be gained into the atmospheric circulation over the North Pacific in the 19th century.

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Appendix 1. Notes on '*Observations Météorologiques, faites à Sitka, sur la côte N.O. de l'Amérique*' by A. T. Kupffer (1846).

1. The daily and monthly station pressures are in different units (French and English (= Russian) half-lines respectively) but the conversion factor is not given. At 13½ °R, when the temperature correction is zero, the data give a ratio of mean daily value to given monthly value of 1·12595. One English half-line = 1/20 inch Hg = 1·6931 mb.
2. The following months contain some unlikely high daytime temperatures: January, February, March, April 1839; January 1840.
   The monthly values have been affected as a result.

Appendix 2. Surface observations at Sitka, Alaska, 1842–67. Background information in *St Petersburg, Annuaire magnétique et météorologique* (1841–45) or *St Petersburg, Annales de l'Observatoire Physique Central de Russie* (1846–67).

Year	Information
1841	(a) Magnetic and geographic [*sic*] station at 57 °3′N, 222° 15′ [*sic*] east of Paris. (b) Detailed observing instructions and tables for temperature correction of barometers, for stations in general. Wind direction is in degrees true, not magnetic.
1844, 1845 1856, 1857 1859, 1860	(a) Sun thermometer readings given. These may assist in the interpretation of some excessively high values recorded at the earlier station in 1839–40.
1844	(a) Psychrometric thermometer broken in November, so that there were no humidity data for most of the month.
1846	(a) Monthly means up to December 1845 given. (b) No Sitka data.
1847	(a) No Sitka data.
1848	(a) Gives May 1847 to December 1848 data.
1849	(a) Volume missing.
1852	(a) Observations for Jan.–June and for 2 Nov.–31 Dec. were made at the normal location at the magnetic observatory on the island called Japonne. (b) Observations for July–Oct. were made at the pharmacy of the hospital of Novo-Arkhangelsk: elevation not given. (c) 25–30 June missing.
1856 and preceding vols	(a) Göttingen time used.
1855	(a) Sitka missing from volume.
1857 onwards	(a) Local time used, with 0 = noon.
1858	(a) Volume missing.
1865, 1866	(a) Location given as 57° 3′N, 137° 49′W of Paris.
1867	(a) Observations ended on 21 October.
1853–58 summary vols. 1859–64	(a) Include 1858 means for Sitka, but not 1855. Normals for 1832–49 are given in 1854 section, where location is given as 57° 3′N, 242° 14′E.

The Groves Family

Mr Nicholas Abbott, who has for the last two years presented the L. G. Groves Memorial Prizes and Awards, has kindly lent us a copy of a privately printed history of the Groves family written by his great-uncle, the late Major Keith Groves, who with his wife Dorothy established the Memorial Fund in memory of their only son, Louis Grimble Groves. The annual presentation ceremony is always attended by a considerable number of the Groves family who take great personal pride and interest in it, and we thought our readers might like to have a short account of Major Groves's background and the connection of his family with the armed services, in particular the Royal Air Force.

The original family surname was Grimble—the late Sir Arthur Grimble, author of 'A Pattern of Islands', was a distant relative—and it was Major Groves's grandfather, born in 1817 as William Peer Grimble, who decided for personal reasons to adopt Groves as a surname. (As a young man he had been sent to study with a firm of analytical chemists in Newcastle and lived in the household of one of the partners, a Mr Groves, who treated him as one of the family.) The Grimbles had for several generations been manufacturers and tradesmen in London, usually as victuallers and distillers, and after various business ventures, including a spell in Australia, W. P. G. Groves in partnership with a Mr A. W. Whitnall established the prosperous brewery of Groves and Whitnall. He had a large family—six sons and six daughters—and the third son, James Grimble Groves (b. 1854) was the father of Major Groves.

James Grimble Groves joined the family business in 1869 as an office boy while continuing his studies at Owens College (which later became Manchester University); it is interesting that at that time a nine gallon cask of the best ale cost 13/–, i.e. 65p. He worked his way up to become Chairman and Managing Director, and also played a full part in public life, becoming a county magistrate, Mayor of Altrincham, and M.P. for South Salford. In 1878 he married Anna Eva Marsland, the daughter of a Manchester physician and his wife who was a descendant of the ancient Lancashire family of Briercliffe. Anna Marsland was an able and well-educated woman with a good business mind, and took a full share in her husband's social, political and philanthropic activities; after his death in 1914 she was elected Chairman of A. V. Roe (then a private company), one of the first women ever to hold such a position. In later life she lived in the Isle of Man where she became Chairman of the Women's Section of the Ramsey Branch of the Royal National Lifeboat Institution. She was a great traveller and was elected a Fellow of the Royal Geographical Society. In old age she used to go on voyages in cargo ships and on one such voyage on a banana boat to the West Indies was lucky to escape shipwreck in a hurricane which caused the loss of the propeller and damage to the rudder.

James and Anna Groves had seven sons and two daughters of whom Major Groves was the fifth son. The youngest son died in childhood, but the others all became men of ability, energy and character who distinguished themselves in various ways.

Listing the children of James and Anna Groves in order of date of birth we have:

1. *William Peer Groves:* Expert on the Far East. Joined Royal Naval Air Service (RNAS) in 1914 and served through the war. Appointed Assistant British Air Attaché to the League of Nations. Became Chairman of Groves and Whitnall, and Hon. Consul for Japan in Manchester.
2. *Robert Marsland Groves:* Joined Royal Navy in 1894, passing out of Dartmouth top of his term. In 1914 Flag Commander to the C.-in-C. Mediterranean Fleet and then appointed Second-in-Command in the RNAS. Commanded Naval Forces at Dunkerque. Became Director of Flying Operations (Navy) and in 1918 Deputy Chief of Air Staff in the new Royal Air Force. Held ranks of Brigadier General, Air Commodore and Captain RN; awarded the C.B., D.S.O., A.F.C. and the American Air Force Cross, and made Officier de la Légion d'Honneur. Played polo, and represented the Navy at fencing. In 1919 appointed to command the Royal Air Force in Egypt and the Middle East, but was killed the following

year in an aircraft accident. A colleague wrote in the *RAF Record* that 'the Air Force owes more to his untiring dash, loyalty and imagination than almost any other man'. His son, Hugh Marsland Groves, also joined the Royal Air Force and retired after the last war as a Group Captain.

3. *James Douglas Groves.* The sportsman of the family. A skilled horseman and first-class shot. Settled in British Columbia where he killed nearly 100 cougars. Served in the Army from 1914 to 1918. His son, Lt.-Col. J. J. D. Groves, presented the Memorial Prizes and Awards in 1978.

4. *Eric Marsland Groves:* Joined the Royal Navy and became one of the earliest submariners. In 1908, following an accident to the petrol supply of his submarine, he by great gallantry saved the lives of the entire crew thereby receiving severe injuries which caused him to be invalided from the Navy.

5. *Eva Muriel Groves:* Married her brother Robert's great friend F. C. Halahan, later Air Vice Marshal Halahan, C.M.G., C.B.E., D.S.O., M.V.O. She herself did much public work.

6. *Keith Grimble Groves:* Barrister. Director (and later Chairman) of Groves and Whitnall. Director of several other companies. Served in the Army during the first world war in France and the Middle East. Mentioned in Dispatches. Did much public work in the Isle of Man including being a J.P. and (during the last war) Second-in-Command of the Second Manx Battalion, Home Guard. Married Dorothy Moore in 1914. Their only son, Louis Grimble Groves, was killed in 1945 when a Sergeant Meteorological Air Observer.

7. *Leslie Gordon Grimble Groves:* Joined the Royal Navy and served in the Battle of Jutland. In business between the wars. Re-enlisted in 1939 and had varied war service, including being trapped for a year in Nice under the Vichy government.

8. *Eileen Norah Grimble Groves:* Married (firstly) Howard Cumming who joined the infantry in 1914 and transferred to the Royal Flying Corps in 1916. Their only son was a pilot in the Royal Air Force in the second world war and lost his life in a bombing mission over Germany.

9. *Colin Groves:* Died in childhood.

The current presenter of the Memorial Prizes and Awards on behalf of the family is Mr Nicholas Abbott whose mother was a daughter of Robert Marsland Groves, Major Groves's brother. Mr Abbott held a short-service commission with the Suffolk Regiment in Germany from 1954 to 1957, and then became a rubber planter in the Far East from 1957 to 1971. After a couple of years on a forestry project in Indonesia, he returned to this country and now works for the Fairbridge Society, a charitable organization which assists single-parent families to settle in Western Australia.

Members of the Meteorological Office probably think of the Groves family only in connection with the L. G. Groves Memorial Prizes and Awards; Major Groves's account makes it clear how wide-reaching their services to their country have been, both in peace and in war.

R. P. W. Lewis

Letters to the Editor

Comments on 'The use of analysis of variance in the assessment of rainfall variability' (by F. M. Courtney, *Meteorol Mag*, **109**, 1980, 268–271).

Courtney writes in the summary of his paper that analysis of variance may be an inappropriate technique for assessing the significance of differences between the catch of two or more rain-gauges. This statement may or may not be true; what is certain is that the evidence given in Courtney's paper does not warrant the conclusion given in his summary. I should like to make the following points:

(a) Courtney's conclusion is based on two analyses of variance of weekly rainfall data from Macclesfield Forest. Using 74 weekly rainfall totals from gauges A (ground level, 260 m altitude) and D (ground level, 365 m) he concludes that the variance ratio (between sites/within sites) was 0·0027 with 1 and 146 degrees of freedom; using 75 weekly rainfall totals from gauges B (canopy level, 345 m) and C (ground level, 335 m), his variance ratio was 0·2750 with 1 and 148 degrees of freedom. (In passing, the formula given in the paper for 'within-site' sum of squares is incorrect; this error may, however, be typographical rather than methodological.*)

The error in Courtney's conclusion stems from the fact that he has analysed his weekly totals as a one-way classification, in which all variation, apart from that ascribable to differences between sites, is classed as 'residual' or 'within-cell' variation. Since he used 74 and 75 pairs of (presumably consecutive, or near consecutive) weekly totals for his two analyses, much of the variation that he ascribes to 'within-cells' is seasonal; this seasonal variation will have grossly inflated his within-cell variance, and will largely account for the small variance-ratios of 0·0027 and 0·2750. His conclusion, that analysis of variance may be inappropriate 'in view of the very large 'within-cell' variations that can arise', is therefore unfounded.

It would be a straightforward calculation to reanalyse Courtney's data as a two-way analysis of variance, in which total variation between weekly rainfall totals would be divided into a part ascribable to differences between sites (as before), another part ascribable to differences between weeks (representing the seasonal factor), and a residual part, which would be a good deal smaller than Courtney's within-cell figure. Alternatively and equivalently, a paired-sample *t*-test would be appropriate, as described in the statistical test quoted by Courtney.

(b) Courtney's description of the analyses of variance technique is unclear. Application of the technique, he says, requires that 'the data should be normally distributed, although this requirement is not absolute and can be relaxed to a certain degree'. He also says: 'a further requirement . . . is that the variables are statistically independent. This requirement is not, however, absolute . . .'. It needs to be made clear that the analysis of variance *per se* requires neither that data should be normally distributed, nor that they should be independent; both assumptions are happily dispensed with where, for example, it is necessary to estimate the magnitudes of variance components. Normality and statistical independence are, however, prerequisites for the use of variance ratio tests of hypotheses.

(c) Newson and Clarke (1976), in the reference given by Courtney, concluded from the analyses of variance that the mean catches of ground-level and canopy-level gauges distributed over the Severn experimental basin did not differ significantly; Courtney interprets this conclusion as being at variance with results of other workers. There is, however, no inconsistency here; Newson and Clarke were making the point that differences in catch between ground- and canopy-level gauges were small relative to 'residual' or 'background' variation, which describes how differences between the catches at different sites within the catchment vary from period to period.

(d) Courtney also says 'a further argument against the use of analysis of variance (and indeed of most relatively sophisticated statistical techniques) with rainfall data is that it involves the artificial delineation of units which are analysed as if they were naturally discrete in time'. I do not believe that analysis of variance is a sophisticated technique; even if it were, it no more requires the artificial delineation of units than any other calculation based on rainfall totals.

---

* Mr Clarke is correct; $\overline{Y}$ was printed instead of $\overline{Yj}$ (Editor).

To conclude, I would perhaps agree with Courtney that analysis of variance is not a perfect tool for the study of rainfall variability; before a tool is judged inappropriate, however, it should first be used correctly.

R. T. Clarke

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Mr Courtney replies:
With regard to the specific points raised by Clarke:
(a) The 74 and 75 pairs of weekly totals were consecutive except that they excluded mid-winter records. It is possible that the variation is 'seasonal', except that no clear seasonal pattern was discernible: merely in some weeks it rained more than in others. Clarke is saying that the 'within-cell' variation is caused by the seasonal effect—I don't dispute that it may be. My point is simply that it exists. I agree that a paired-sample *t*-test would be appropriate—this was stated in the paper.
(b) Clarke's point is unclear to me. One *can* use any data with any statistical test, but that is not to say that it is correct to do so. The point I was trying to make was that the analysis of variance is usually considered to be a parametric technique. A parametric technique requires that data be normally distributed.
(c) This is an elaboration of the explanation in Newson and Clarke's original paper, which makes an interesting new point.
(d) I do not think we disagree. The truth of my original point that 'analysis of variance with rainfall data involves the artificial delineation of units which are analysed as if they were naturally discrete in time' does not appear to be in dispute. The significance of this point is a matter for subjective judgement and I agree with Clarke that the problem is not specific to analysis of variance.

Clarke's final statement implies that I used analysis of variance incorrectly. Unfortunately, as this correspondence amply demonstrates, 'correct' use of statistical procedures is not an absolute quality. On the 'scale of incorrectness' my use may have been lower down than his, but Clarke's reaction suggests that my original point was well made: analysis of variance and rainfall data are not happy partners.

Comments on 'Computation of vapour pressure, dew-point and relative humidity from dry- and wet-bulb temperatures' (by G. P. Sargent, *Meteorol Mag*, **109**, 1980, 238–246).

The article by Sargent (1980) on the computation of vapour pressure, dew-point and relative humidity deals with only one of the factors contributing to their 'accuracy'. In fact the calibration function aspect that Sargent deals with can be made increasingly 'accurate' but formulae with 10-digit coefficients in sixth-order polynomials are unnecessary and illusory with regard to overall accuracy.

Although modern forms of electrical sensing can give additional sources of error (Revfeim and Jordan, 1976) the simple thermometer reading considered by Sargent has three error components: the resolution of the temperature reading, the calibration function and the spatial variation of the temperature field represented by the sampled reading(s).

It is easier to consider the contribution of resolution errors using the approximate calibration formula

$$e_w = \exp\{7{\cdot}1-2{\cdot}5(1{\cdot}46-t/100)^2\}$$

(Revfeim and Jordan, 1976). Assuming t to be an observation of an observable value T with resolution error δT, a Taylor expansion gives

$$\begin{aligned} e_w(t) &= e_w(T+\delta T) \\ &= e_w(T) + \delta T\, e'_w(T) + \text{higher-order derivatives.} \end{aligned}$$

Now in the exponential form of calibration function the first derivative of e_w is $(1{\cdot}46-t/100)e_w/20$. Hence the relative error in e_w due to a resolution or reading error, of δT is $\delta T(1{\cdot}46-t/100)/20$. For thermometer readings δT will be of order 0·1 °C which gives relative errors between 10^{-2} and 5×10^{-3} over the temperature range −40 °C to +50 °C. Compare these relative errors with the calibration function relative error of order 10^{-5}, as calculated by Sargent, and one sees the scope for using simpler calibration functions.

The third error component from variability in the temperature field reduces the contribution from the calibration function to the point of being negligible unless it has a relative error greater than order 10^{-1} (Revfeim, 1978).

The conclusion is that in real applications, calibration functions do not need to be much more complicated than the simple exponential quadratic form given above. In particular since climatological records of vapour pressure are only maintained to 3-digit resolution (Ricketts, 1980) it would be unreal to use formulae with 10-digit coefficients. To misquote Kronecker one is tempted to say 'God created the numbers; man devised the polynomials; all the rest is the artificial accuracy of computers'.

K. J. A. Revfeim

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Sargent, G. P. 1980 Computation of vapour pressure, dew-point and relative humidity from dry- and wet-bulb temperatures. *Meteorol Mag*, **109**, 238–246.

Mr Sargent replies:

Revfeim goes to some trouble to show what one intuitively knows, namely that it is a waste of time and effort chasing accuracy in one part of a system if a similar accuracy is not achieved throughout the rest of the system. With a simple dry- and wet-bulb psychrometer the errors due to reading, exposure,

ventilation etc. are of such magnitude as to make the use of very precise approximations to the saturation vapour pressure nonsensical and it was for this reason that simpler, less accurate approximation formulae were included in the article.

However, there may well be occasions when work is being done under very carefully controlled conditions with very accurate equipment when a high degree of precision is required. The Goff–Gratch formulations are themselves only approximations to the 'truth', albeit generally accepted as the best available, and if it is desired to make the computation simpler the closer one can get to these 'best approximations' the better. It is for the user to decide what degree of accuracy is necessary in a particular set of circumstances, but it is far easier to degrade accuracy by truncating coefficients than to regain it once the discarded digits are lost.

Reviews

Ball lightning and bead lightning: extreme forms of atmospheric electricity, by James Dale Barry. 150 mm × 230 mm, pp. x + 298, *illus*. Plenum Press, New York, 1980. $29.50 (+ 20% outside USA).

Although descriptive accounts of lightning exist in the scientific literature, there are a number of important gaps in our understanding of the physics of the discharge. The rarity of the natural phenomenon, its high energy density and the inaccessibility and rather small scale of many of the important processes, combine to present great problems for any experimental, and hence theoretical, study. These difficulties have been so severe for the subset of discharges known as bead and ball lightning, as to place them on the fringes of myth and illusion.

This book is the second in recent years to attempt a review of the evidence for ball lightning; the first was Singer's *The nature of ball lightning*, Plenum Press, 1971. Both authors, faced with a dearth of planned scientific observation, have been forced to make maximum use of serendipity. Unfortunately, the careful investigation of 200 years of accidental glimpses of the phenomena demands considerable scholarship, but the result makes rather tedious reading. With the advantage of Singer's exhaustive review before him, Barry has avoided a detailed description of the vast number of narrative reports and discussions of ball lightning and concentrated his attention on some specific topics. Nevertheless, a substantial fraction of his book is taken up with the minutiae of photographic evidence, much pored over in the past and now listed, cross-referenced, described again, and in most cases discarded as of doubtful validity and/or of little value. The bibliography occupies a third of the book. Doubtless this will be of use to a small number of researchers, but to most readers it is likely to remain a formless and indigestible list.

The author's expressed intention is to emphasize the physical aspects of the chosen discharge phenomena such as their dimensions, luminosity, motions, form of decay, and their effect on their observers and environment. From these he attempts to deduce properties such as temperature, mass and energy density. This is done very superficially for bead lightning but a comprehensive statement is provided of the observed and inferred properties of ball lightning. Much is made of the author's previously published thesis that the deduced energy density of ball lighning is normally distributed and that this suggests a single common form for the object. Neither the adopted methodology for this analysis nor its conclusions are persuasive.

The major remaining section of the book is devoted to a discussion of laboratory or field studies which have produced small-scale luminous phenomena. These include localized burning processes requiring combustible gases, the thermal emission of metal-vapour residues and the relaxation of molecules from metastable states with the emission of visible radiation. The treatment of the physical processes involved is always incomplete and usually superficial. Their relevance to bead and ball lightning remains obscure. The question of containment is avoided or, where identified, as in the case of the magnetic pinch, described only in qualitative terms. This is the closest Barry comes to an evaluation of the many theories which have been proposed to account for bead and ball lightning.

One is left with the impression of some careful and diligent detective work brought to bear on a difficult subject, but without great authority or insight. It is not possible to recommend ownership of the book to the interested non-specialist, but a few hours' reading of Chapters 3, 4 and 7 of a library copy will provide a useful guide to some current thinking.

P. Ryder

Agro-meteorology, by J. Seemann, Y. I. Chirkov, J. Lomas, and B. Primault. 240 mm × 170 mm, pp. vii + 324, *illus.* Springer-Verlag, Berlin, Heidelberg, New York, 1979. Price DM 98, US $53.90.

Agricultural yields and profits in the developed world depend upon the weather. Our very survival is determined by weather in some parts of the developing world. The sorts of agriculture that are possible at a particular place vary with climate. These facts have forced the farmer and the agricultural scientist into a wary determination to outface and overcome the weather and to understand the relations between weather, climate and agriculture.

Both climate and weather are difficult terms to define. Lamb has defined climate as comprising 'the totality of weather experienced at a given place' and has suggested that we may think of it as 'long range meteorology'. In agricultural meteorology we can think of weather as being the real atmospheric conditions experienced in a particular place at a particular time. The actual yield of a crop or the production of an animal will depend, in part, upon the direct influence of weather on the biological system and upon the influences of weather on pests, diseases and the timeliness of farming operations. Clearly, these relationships are complex and our understanding is, therefore, usually partial and often inaccurate.

However, there is a substantial body of operational achievement, including the estimation of the irrigation requirement of a crop and the forecasting of the development of a disease. There are also growing, but still insufficient, intellectual achievements, such as our understanding of the relationship between solar irradiance, canopy size and structure, photosynthesis, and the rate of growth of a crop. It was inevitable that the operational aspects have been developed by the national organizations for weather forecasting. It is unfortunate that, in general, these national organizations have not been allowed, or perhaps have not been able, to play a larger part in the intellectual development, which has generally fallen to research institutes and university departments.

This book shows the catastrophic consequences of the separation. It is the result of an initiative taken at a meeting of the Commission for Agrometeorology of the WMO. There are two parts to it, one dealing with the 'Physical and Meteorological Principles of Agrometeorology' and the other dealing

To face page 171

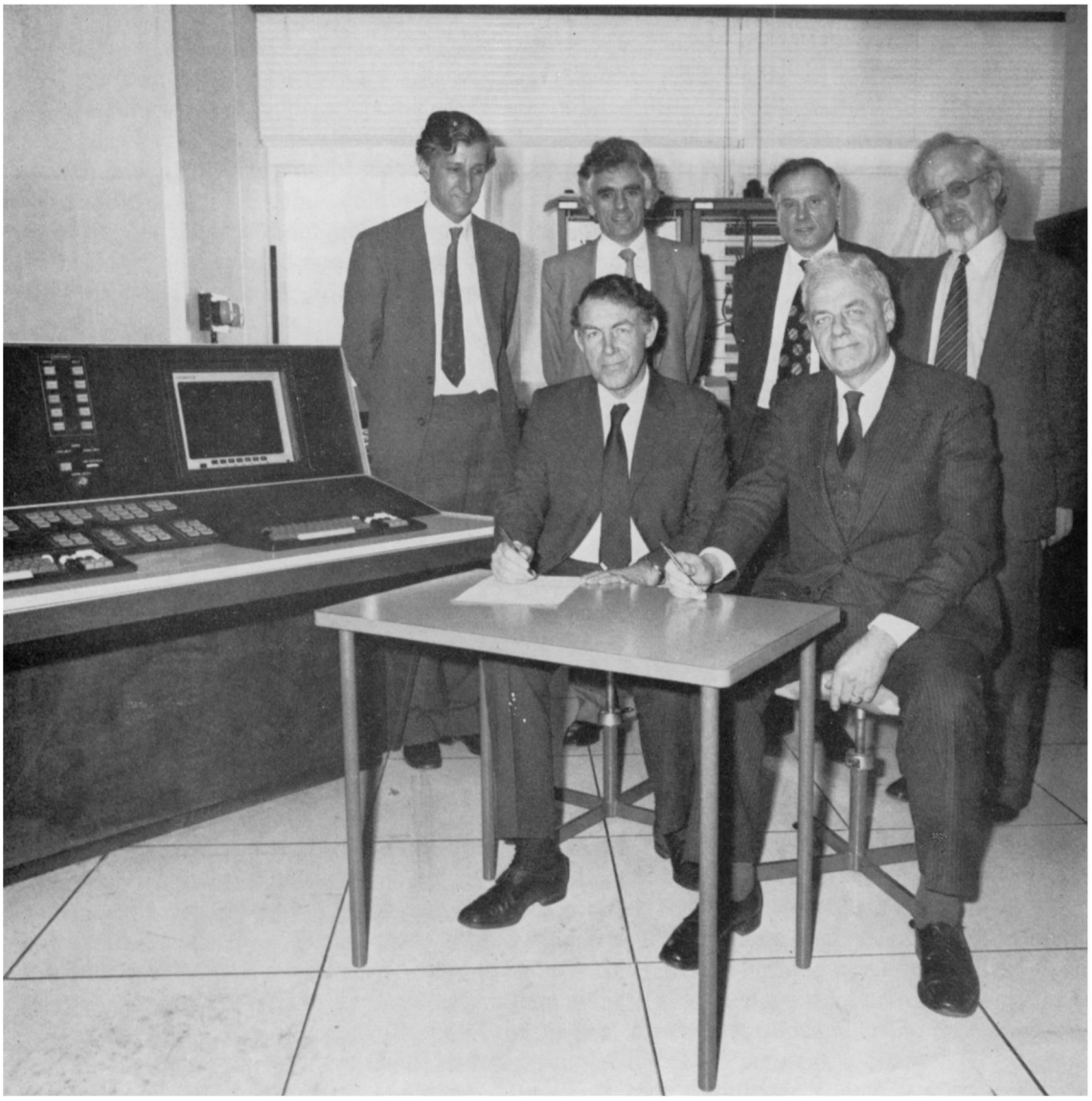

Plate I. The Main Enhancement to the Bracknell automated Meteorological Telecommunication Centre becomes operational—7 February 1981.
Seated, left to right: Mr G. Dootson, Ferranti Ltd and Mr K. W. Hathaway, AD Comms 2. Standing, left to right: Dr D. N. Axford, AD MetO(TC), Mr S. Thomas, Ferranti Ltd, Mr P. Caswell, Ferranti Ltd and Mr C. E. Goodison, Met O 5 (See facing page).

with 'Applied Agrometeorology'. The first manages to omit much that is important, such as any real consideration of the exchanges between crop and atmosphere, and to include the trivial, such as a description of a 'Weathercock without Vane (Direction)'. The authors do not separate agricultural climatology from agricultural meteorology nor do they use the scale of the processes involved in the exchanges of energy and matter to organize and simplify.

The second part, called 'Applied Agrometeorology', follows directly, without any considerations of biology, agriculture or the study of systems. It becomes an unavoidable still-birth, devoid of any possibility of intellectual life. Again, it is incomplete. In places, as in the chapter on irrigation, it is a pretentious and unscholarly assemblage of an ill-selected number of publications. In other places, as in the chapter on the 'Epidemiology of Insects and Other Diseases' (*sic*), the authors avoid all discussions of the practical and useful.

I guess that the book was written in response to the recognition of real needs, intellectual and practical. I guess that it was written, perhaps under pressure, by busy men. I am sorry that it was written and hope that it does not represent the present form or future direction of agricultural meteorology.

J. Elston

Notes and news

The Ferranti enhancement to the Bracknell automated Meteorological Telecommunication Centre (Met TC) becomes operational

The technical transfer of the Ferranti enhancement to the Telecommunication computer complex in the Bracknell Regional Telecommunication Hub took place on Tuesday 7 February 1981 (see Plate I). This Argus 700S dual computer system increases Bracknell's capacity for handling medium-speed lines from 7 to 20. Up to 13 new channels are provided at speeds ranging from 1200 bits per second to 9600 bits per second, allowing a maximum total throughput of 10×10^6 characters per hour. The Bracknell automated Met TC is now one of the most powerful message-switching complexes in the WMO Global Telecommunication System. At present there are medium-speed connections to Washington, Paris, Offenbach, De Bilt, Dublin, Oslo, and the European Centre for Medium Range Weather Forecasts, and plans are in hand for links to Belgium and Iceland in the near future.

The Ferranti Argus 700S computers are built with fast emitter-coupled logic. There is a main store of 96K 16 bit words (32K words of fast semiconductor memory and 64K words of ferrite core memory). There are three input/output sub-processors, two 80 megabyte moving-head discs and two fast access 2 megabyte head-per-track discs, with off-line spares of each type. Two visual-display units are mounted in a control-and-status console for control and reject operation, and push buttons control and display the status of the various channels and peripherals. The software consists of a comprehensive set of 130 application modules directed by a basic Ferranti operating system. These application modules carry out the specialized message-switching functions, such as queuing, scheduling, and statistical monitoring, required of the enhancement.

The contribution of marine meteorology to economic development

(based on a WMO Press Release)

The tourist enjoying the sunshine on the beach outside his fine new hotel and the port construction engineer exposed to cold and damp working conditions may seem to have little in common. Yet each is equally dependent upon weather and sea conditions, and the chances are that the changes which each would strongly dislike, rain, wind, swell and so on, would largely have their origins in ocean areas. The seas and oceans of the world cover over two-thirds of the surface area, and if the land masses were to be skimmed off at sea level, they would be easily accommodated in the ocean space available. This simplification emphasizes the dominance and potential power of the oceanic regions, and it is natural that mankind became aware many years ago of the need for scientific study of all aspects of the oceans and their behaviour. To ignore the results, often hardwon, of those long years in a world where the problems of space, natural resources and social and economic development require unstinting attention would be unsound.

Fundamental to most sciences is measurement; for the oceans, this means observations, both visual and instrumented, and, equally vital, recording of this information. Despite centuries passing since man first put to sea, few data were collected and rarely combined with other information. It was as late as 1853 before the first international agreement upon some uniformity of observation, and agreements upon exchange of data were achieved. In the subsequent years, the use of ships to collect routine and regular observations, whenever at sea, has grown tremendously, there being now some 7000 registered voluntary observing ships. The volume and density of data over the oceans, however, remains very variable—ships have their 'roads' too, and in many regions the information level is still sparse. Whilst this is the position for observations made on the surface of the sea, the situation with regard to underwater observations is very much worse. These data have only been collected routinely for some 30 years apart from scientific cruises and research, and rapid growth is now being supported. In order to apply this resource of data and the applications derived from it to development requirements, it is helpful to appreciate the foundation of the support. Today's marine meteorological and oceanographic data banks use high-speed computers to accommodate their holdings, and data are being collected steadily every day. Even though more are needed, the important feature is that many and varied services are available based on these data, and more and more highly specialized support to engineering, industrial, and social and economic development is being introduced.

As for more information services, one of the strengths is the strong liaison between the service and the user which is created, and that a contribution is made by both sides. In the same way that mariners obtain radio weather forecasts which have made use of information and weather reports from the mariner in their production, so any development study needs to be assisted in determining what further information or data must be obtained. It is only against the background of already acquired knowledge that proper and reliable judgement can be made. The early explorations and development for oil and gas in the North Sea region formed a good example of this situation. In a relatively small sea area, not far offshore with large amounts of data available from land station records, the operators were surprised to find that the detailed information and support they needed was not available. The reason, valid for many developments to come elsewhere, was that such detailed questions, as, for instance, when will the waves next be less than two feet for 36 hours, or what is the maximum rate of wind direction shift and what force will this have on a buoy, had never been posed against real operations. That the marine meteorological services were able to find the answers and establish regular support services to deal with many other complex problems as they arose, all realized in full association with the operators themselves, shows the benefits which can be achieved.

The contribution which can be made to a coastal or estuarine development is also significant. A study of past marine meteorological data can throw light on many development aspects. With computer models, hindcast techniques and forecast programming, considerations of potentially critical factors can be completed. The risks to a coastline of flooding by surge or storm effects, and the engineering requirements of dykes, harbours, barrages, the frequencies of high or storm winds for cabling, high structures and the maximum 100-year values, the probable pattern and effects of pollution in a sea or coastal chemical plant development, the choice of sites for new developments, the architectural limits and land reclamations, these are all matters for which marine meteorology and related services provide an early basis, and should play a full part in planning and studies and, later, be closely involved in continuing support.

Economic developments carry many implications for trade and food production. The marine meteorological services of today provide world-wide support to international shipping and to fishing industries. Thanks to the World Weather Watch (WWW), the international exchange of data, including all reports from ocean areas, has been established. This today also includes data from satellites scanning the oceans, aircraft and buoys at sea, combining both surface and upper-air information for the marine meteorologists to use in producing warnings, forecasts and detailed special support, as required. The costs of fishing operations, for instance, must be minimized, and services aid trawlers to choose good fishing grounds, know their predicted movement, and avoid weather which would interrupt fishing. As the need for natural resources to be carefully used increases, the support services will be called upon for more specialized and detailed advice, both for planning and operations, as is now the case for the energy industry offshore.

The capability of the WWW Global Data-processing System (GDPS) for ocean-wide analysis and forecasts, with the use of numerical prediction techniques on the marine meteorological data originating in the WWW Global Observing System (GOS) and collected by the WWW Global Telecommunication System (GTS), which supports all these marine activities, also provides for a further service of economic importance. This is 'ship routeing' for long ocean passages of passenger and cargo vessels. Vessels may need the quickest total passage time, avoiding head seas, strong winds and currents and icebergs, may need calm and dry conditions throughout to protect cargo, or to be certain to reach a port at a specific time. Specialist marine meteorological ship-routeing officers evaluate storm movements and predicted paths, waves, swell and surface currents, as they affect the particular vesssel concerned, and knowing its physical characteristics, and knowing the ship's requirements, advise the ship's master before and during the voyage. Savings of one to two days per vessel per passage can be obtained and the total benefit to the costs of operating a major vessel are highly significant. The growth of world trade can be expected to place greater demands upon this type of service over the next decade.

It is clear that the applications and services now being provided from the meteorological and oceanographic data arising from past and present marine activities are making a positive contribution to many aspects of social and economic development. In the future, these services will need to be certain that they can expand and become more specially tailored to meet demand, as the economic needs of the world are themselves established. With the ever-growing use of automated observation systems at sea and in the atmosphere, the advances foreseen in data-processing technology, telecommunications, and the techniques of prediction, there should be no doubt that marine meteorology will do so. As the oceanic regions will no doubt be unlikely ever to give up their majestic authority, so mankind must continue to look to the sea, but must ensure they have the fullest understanding and use it to the greatest possible extent.

THE METEOROLOGICAL MAGAZINE

No. 1307 June 1981 Vol. 110

CONTENTS

Page

A mesoscale forecast for 14 August 1975—the Hampstead storm. M. J. Bailey, K. M. Carpenter, Laurina R. Lowther and C. W. Passant 147

Early meteorological observations for Sitka, Alaska. D. E. Parker 161

The Groves Family. R. P. W. Lewis 164

Letters to the Editor 165

Reviews

Ball lightning and bead lightning: extreme forms of atmospheric electricity. J. D. Barry. *P. Ryder* 169

Agro-meteorology. J. Seemann, Y. I. Chirkov, J. Lomas, and B. Primault. *J. Elston* .. 170

Notes and news

The Ferranti enhancement to the Bracknell automated Meteorological Telecommunication Centre (Met TC) becomes operational 171

The contribution of marine meteorology to economic development 172

NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

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